Bank Interventions and Firm Innovation: Evidence from Debt Covenant Violations[*]

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Abstract

We examine the effect of bank interventions on corporate innovation and firm value via the lens of debt covenant violations. Bank interventions have a significantly negative effect on innovation quantity, but no significant effect on quality. The reduction in innovation quantity is concentrated in innovation activities that are unrelated to the violating firm's core business, leading to a more focused scope of innovation investment and ultimately an increase in firm value. Human capital redeployment appears a plausible underlying mechanism through which bank interventions refocus innovation scope and enhance firm value. Our paper sheds new light on the real effect of bank financing.

JEL Classification: G21, G32, G34, O31

Keywords: Bank interventions, innovation, covenant violations, human capital redeployment, firm value

1. Introduction

To what extent do banks affect firm innovation and hence firm value? This question is important because technological innovation is vital for a country's economic growth (Solow 1957; Romer 1986) and a firm's long-term competitive advantage (Porter 1992).[1] Meanwhile, banks are probably the most heavily regulated financial institution in the U.S. in the past century, and their behavior can be altered by financial market regulations and security laws. Hence, this question is of particular interest and relevant to firm stakeholders, regulators, and policy makers.

However, the answer to the question is not straightforward because banks are generally passive investors. Unlike active equity investors such as venture capitalists or hedge fund activists, banks do not get involved in a firm's daily operations when the firm is in good financial conditions. Therefore, it is difficult to gauge (if any) the direct effect of bank financing on firm innovation. However, upon a firm violating debt covenants, control rights are shifted from equity holders to creditors who are able to affect a firm's innovation policy through their threat of terminating the loan or accelerating the debt principal. All of their actions are able to influence a firm's various policies, including its innovation policy, during debt contract renegotiations (Sufi 2007).[2] In this paper, we use an observable event, debt covenant violations, to evaluate the effect of bank interventions on firm innovation. We further explore the valuation effect of bank interventions on violating firms through the innovation channel.

We are not the first to explore the important role that debt-holders play in the governance of firms. A growing body of literature emphasizes on the importance of active creditor control outside of bankruptcy and payment default. For example, creditors with financial interests tied with a firm's cash flow have an incentive to influence its operations. Daniels and Triantis (1995) and Baird and Rasmussen (2006) provide anecdotal evidence suggesting that creditors have an incentive to play a role beyond bankruptcy. Ivashina et al. (2009) show that bank lending intensity and bank client network facilitate takeover attempts. Recent studies (for example, Chava and Roberts 2008; Roberts and Sufi 2009; Nini, Smith, and Sufi 2009, 2012; Ozelge and Saunders 2012) document that creditors assert substantial influence on corporate control and governance by

[1] According to Rosenberg (2004), 85% of economic growth could be attributable to technological innovation.

[2] Specifically, upon covenant violations, banks have the ability to accelerate debt principal, increase the loan rate, and terminate unused credit line facilities (Sufi 2007). Although creditors often waive the violation, the potential threat associated with these activities allows banks to exert significant influence over the firm. Note that while more non-bank financial institutions (for example, mutual funds and hedge funds) participate in the syndicated loan market in recent years, lead lenders that are responsible for negotiating and renegotiating loan contract terms are exclusively banks. Hence, we use the words "creditors", "lenders", and "banks" interchangeably in this paper.

taking active actions to protect their claims when firms breach their debt covenants, for example, reducing capital expenditures, debt issuing, acquisition spending, and shareholder payouts, demanding better reporting and liquidity management, pushing for replacement of top executives, and so on. Our paper contributes to this emerging literature that highlights the role debt-holders play in the governance of firms by studying firm innovation. Our focus of technological innovation allows us to provide a number of new insights beyond these existing studies.

First, innovation has many unique features that are distinct from conventional investment such as capital expenditures and acquisitions. As Holmstrom (1989) points out, innovation is a long-term, risky, and idiosyncratic investment in intangible assets that requires much exploration of unknown methods, while conventional investment is the exploitation of well-known approaches. Hence, investing in innovation is typically lack of tangible collateral and its payoff structure is more risky. As a result, debt contracting problems could be more difficult for financing and motivating innovation than for conventional investment. For example, there has been an emerging literature showing that economic factors affect capital expenditures and innovation in substantially different ways.[3] Therefore, while existing studies show that bank interventions lead to a cut in conventional investment such as capital expenditures and acquisitions, it is unclear *ex ante* how a firm's innovation changes after the covenant violation.

Second, we use patent information from the National Bureau of Economic Research (NBER) database to capture innovation output.[4] We observe both the number of patents a firm generates and the number of citations these patents receive in the future. Hence, patent information allows us to explore the effect of bank interventions on not only the quantity but also the quality of innovation output by a violating firm. This unique feature makes technological innovation an outcome variable that is superior to those examined in previous studies, because one cannot easily judge the change in the quality of capital expenditures, acquisitions, payout, and so on, despite the reduction in their quantity.

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[3] For instance, while the IPO literature documents that going public allows firms to raise capital to increase capital expenditures, Lerner, Sorensen, and Stromberg (2011) show that private instead of public ownership promotes innovation because private ownership allows more failure tolerance from investors (Manso 2011). Existing studies argue that financial analysts reduce information asymmetry and the cost of capital, which in turn increases capital expenditures (for example, Derrien and Kecskes 2013). However, Benner and Ranganathan (2012) and He and Tian (2013) find that analysts hinder innovation because they impose short-term pressure to meet earnings target on managers. Many studies show that stock liquidity facilitates information production and enhances a feedback effect that allows managers to learn from informative stock prices. Hence, stock liquidity increases capital expenditures. However, Fang, Tian, and Tice (2014) find that high stock liquidity impedes innovation because stock liquidity increases a firm's takeover exposure and attracts short-term institutional investors.

[4] Our use of patenting to capture firms' innovation output has become standard in the innovation literature (for example, Acharya et al. 2013; Aghion et al. 2013; Nanda and Rhodes-Kropf 2013).

Finally, we are able to observe the scope of a firm's innovation, which allows us to explore the "focus" of the innovation projects that are cut or persist after a violation. The NBER database provides detailed classifications of each patent's technology class that can be mapped to standard industry classifications. Hence, we can construct proxies that capture a firm's innovation scope and compare it against its core business. By linking this analysis to the stock abnormal returns upon patent grants (namely, economic value of patent) after the violation, we are able to examine direct channels that allow bank interventions to affect firm value. This test is not possible in previous studies because one does not easily observe the scope of capital expenditures.

One reasonable concern is that while debt covenants restricting capital expenditures are quite common in lending contracts, the contracts generally do not contain covenants related to firms' innovation activities, although some covenants indeed do. As a result, what is the lever that the bank uses to alter a firm's innovation following covenant violation? The reason that banks can push firms to change their innovation activities after violations is that, once control rights are shifted to banks, they could use the threat of terminating the loan facility or accelerating the principal to influence borrowing firms' investments policy. Because innovative firms are generally lack of tangible assets and the innovation process is long, risky, and idiosyncratic, these firms are particularly vulnerable to the enhanced bargaining power of their lenders. As a result, banks could force violating firms to switch their long-term investments in innovation to less risky, more short-term ones that can generate more stable cash flows, which naturally results in a cut in innovation.

Based on the existing theoretical literature, we postulate that bank interventions reduce firm innovation. However, two strands of underlying theories predict that reduction in innovation could be due to either bad reasons or good reasons. The first strand of theories argues that the reduction in innovation output due to bank interventions leads to a drop in firm value, and this is because of at least two reasons. First, due to the payoff structure of creditors (namely, creditors do not share upside returns when innovation succeeds but suffer from downside losses when innovation fails), Stiglitz (1985) points out that a debt contract is not well suited for innovative firms with uncertain and volatile returns. Second, there is a hold-up problem associated with bank financing. Because banks collect soft information about the firm (such as the underlying quality and prospects of its innovative projects) that the firm cannot easily communicate to other investors, banks have bargaining power over the returns from the firm's investing in innovative projects, once the innovation process has started. Hence, as argued by Hellwig (1991) and Rajan (1992), powerful banks frequently stifle innovation by extracting informational rents. Based on these theoretical arguments, we expect firm innovation output drops after bank interventions. To the extent that innovation output is positively associated with firm value (Hall, Jaffe, and Trajtenberg 2005), the reduction in innovation output leads to a drop in firm value. We term this view as the "value-destroying hypothesis."

Alternatively, bank interventions could reduce firm innovation for good reasons because they mitigate managerial agency problems, as argued by another strand of theories. Due to conflicts of interest between managers and shareholders, managers may overinvest in innovation to enjoy their private benefits from such activities. Scharfstein and Stein (2000) argue that specialized investment, such as investment in innovation whose process is long, risky, and idiosyncratic, effectively entrenches the management. In addition, managers with career concerns who want to "grandstand" (for example, Gompers 1996) could overinvest in innovative projects that may not necessarily best serve shareholders' interest. Finally, overconfident CEOs may overinvest in innovative projects even if their firms are in non-innovative industries, which do not improve firm value (Hirshleifer, Low, and Teoh 2012). After banks step in upon covenant violations, they could help curtail excessive investment in innovative projects that are tangent to the firm's core business and hence are value-destroying. If this argument is supported, we expect that firm innovation drops after bank interventions but this reduction in innovation leads to an increase in firm value. We refer to this argument as the "value-enhancing hypothesis." In this paper, we test these two hypotheses by examining the effect of bank interventions on firm value.

Our baseline ordinary least squares (OLS) results suggest that bank interventions are significantly negatively related to firms' patent quantity. However, bank interventions do not significantly affect patent quality captured by the number of citations per patent. An important concern of our OLS analysis is that bank interventions due to covenant violations are likely endogenous. Unobservable firm heterogeneity correlated with both covenant violations and firm innovation could bias our results (namely, the omitted variable concern). Meanwhile, firms with low innovation potential (and therefore low future innovation output) may be fundamentally low quality firms and therefore are more likely to violate covenants (namely, the reverse causality concern). To address the identification issue and establish causality, following Chava and Roberts (2008), we use a regression discontinuity design (RDD) approach.

The RDD relies on "locally" exogenous variation in covenant violations generated by the distance to the covenant threshold. This empirical approach essentially compares the innovation variables of firms that just violate covenants to those that barely avoid violating covenants. The RDD is a powerful and appealing identification strategy because for these firms falling in a narrow band of the distance to the covenant threshold, the covenant violation is very close to an independent, random event and therefore is unlikely correlated with firm unobservable characteristics. Our results from the RDD suggest that bank interventions due to covenant violations lead to a significant drop in patent counts after the violation, but no significant decline in innovation quality. Overall, our RDD results suggest that bank interventions appear to have a significantly negative, causal effect on innovation quantity, but not a significantly effect on quality.

We further explore how our main results vary in the cross section with different degrees of managerial agency problems. If the reduction in patent counts upon covenant violations is due to the shift in control rights from shareholders to creditors, which alleviates the agency problems between managers and shareholders, we expect firms that are subject to more severe agency problems to be affected more by bank interventions. Consistent with the conjecture, we find that post-violation declines in patent counts are significant only in firms that are subject to managerial agency problems to a larger degree, namely, firms that have no credit ratings, have no prior relationship with their lead lenders, and borrow from a small syndicate of lenders.

We next attempt to answer a "bottom-line" question regarding the economic value implications of reductions in innovation output caused by bank interventions, and distinguish the predictions proposed by two strands of theories discussed earlier, namely, the "value-destroying hypothesis" vs. the "value-enhancing hypothesis". We first show that the reduction in patent counts is concentrated in innovation activities that are unrelated to a firm's core business. However, patent production related to a firm's core business remains unchanged after covenant violations. As a result, firms have a more focused scope of innovation output after bank interventions. To the extent that innovative activities unrelated to a firm's core business could be due to managerial agency problems (namely, arising from managers' private benefits) and could be out of managers' expertise and hence value destroying, a more focused innovation scope should enhance firm value.[5] We confirm this conjecture by showing that patents related to a firm's core business are

[5] See, for example, John and Ofek (1995) and Daley, Mehrotra, and Sivakumar (1997) for a similar argument in the context of spinoffs and asset sales as well as Berger and Ofek (1995), Comment and Jarrell (1995), and Lang and Stulz (1994) in a more general context of corporate focus.

associated with a greater firm value than unrelated patents. Overall, our evidence is consistent with the "value-enhancing" hypothesis.

Finally, we show that human capital redeployment appears an underlying mechanism through which creditors curtail overinvestment in innovative projects that are unrelated to the violating firm's core business and hence enhance firm value. We find that leavers (individual inventors who leave the firm after the violation) of violating firms produce fewer patents that are related to the firm's core business than stayers (inventors who stay in the firm after the violation) and new hires (inventors who join the firm after the violation), suggesting that violating firms actively replace inventors who produce fewer related patents with inventors who produce more related patents. In addition, stayers of violating firms generate a larger fraction of patents that are related to the firm's core business post-violation than those of non-violating firms.

Overall, our results have important implications on optimal capital structure for corporate innovation. Existing literature finds that there is a negative association between firm leverage and R&D expenditures, and hence concludes that debt financing is not suited to firms with intensive innovation activities (see Hall and Lerner (2010) for a survey of this literature). This view is especially true for young, unlisted startups in which managerial agency problem is less severe but achieving and maintaining technological advantages over competitors through innovation is more critical to a firm's survival and success. Instead of focusing on the extensive margin as the prior studies do, i.e., how would a firm choose between equity and debt when financing its innovative project, our paper contributes to the literature on the intensive margin. Specifically, we explore, conditional on a firm having bank loans in its capital structure, what is the impact of covenant violations on its innovative behavior? Our findings supplement existing studies by showing that in mature firms in which innovation is used as a tool to entrench managers and hence managerial agency problems are more severe, once control rights are shifted to banks, they are able to mitigate managerial agency problems by refocusing a firm's innovation scope and cutting unrelated innovation activities, which ultimately enhances firm value. Our paper is consistent with a recent emerging strand of literature that shows the importance of bank financing on corporate innovation (for example, Amore et al. 2013; Chava et al. 2013; Cornaggia et al. 2015). As a result, it is clear that our paper is not about how to best finance innovation, nor is about firms focused particularly on innovation. Instead, our paper focuses on firms that use bank loans for various reasons and also do innovation.

The rest of the paper is organized as follows. Section 2 discusses the related literature. Section 3 describes sample selection and variable constructions, and reports summary statistics. Section 4 presents the baseline results and addresses identification issues. Section 5 discusses economic value implications of bank interventions. Section 6 concludes.

2. Relation to the existing literature

Our paper contributes to two strands of literature. First, our paper is related to a growing literature on the role played by creditors in the governance of firms and the effects of covenant violations. Chava and Roberts (2008) find a decline in firm investment after the violation and this reduction is more pronounced in firms in which agency and information problems are more severe. Roberts and Sufi (2009) focus on the effect of covenant violations on capital structure and find that net debt issuance drops significantly and the decline is persistent following covenant violations. Nini, Smith, and Sufi (2009) show that 32% of private credit agreements contain an explicit restriction on the firm's capital expenditures and these restrictions cause a reduction in firm investment in tangible assets. Nini, Smith, and Sufi (2012) find that covenant violations are followed by a decline in acquisitions and capital expenditures, a reduction in leverage and payouts, and an increase in CEO turnover. They also show that firm operating and stock price performance improve after bank interventions. Falato and Liang (2016) show a sharp drop in employment following covenant violations. Billett, Esmer, and Yu (2018) find that covenant violations affect rival firms' product market behavior. Our findings that creditors help mitigate investment distortions in innovation and ultimately enhance firm value are consistent with the implications of existing literature, for example, Chava and Roberts (2008) and Nini, Smith, and Sufi (2012), which show that bank interventions benefit firm performance.

Second, our paper adds to the fast growing literature on finance and innovation. Theoretical work from Holmstrom (1989) argues that innovation activities mix poorly with routine activities in an organization. Aghion and Tirole (1994) suggest that the organizational structure of firms matters for innovation. Manso (2011)'s model shows that the optimal contract that motivates innovation involves a combination of tolerance for failure in the short run and reward for success in the long run.

Empirical evidence suggests that various equity market environment and characteristics affect managerial incentives to innovate. Specifically, a larger institutional ownership (Aghion, Van Reenen, and Zingales 2013), private instead of public equity ownership (Lerner, Sorensen, and Stromberg 2011), corporate venture capital (Chemmanur et al. 2014), and "hot" rather than "cold" markets (Nanda and Rhodes-Krpof 2013) alter managerial incentives and hence motivate managers to focus more on long-term innovation activities.[6] However, existing studies have largely ignored the role played by credit market investors. Although an emerging literature examines how banking deregulation and competition affect innovation (for example, Amore et al. 2013; Chava et al. 2013; Cornaggia et al. 2015), there is no study that provides direct evidence on the effect of bank financing on firm innovation. We contribute to this line of research by filling in this gap.

Our paper is related to Atanassov (2016) who shows that arm's length financing (equity and public debt) is positively related to innovation while relationship-based bank financing is negatively related to innovation. Our paper advances this line of inquiry in three important dimensions. First, using covenant violations that shift control rights from equity holders to creditors, we pin down the direct effect of bank interventions on innovation rather than relying on a firm's loan stock to infer the effect of relationship-based bank financing. Second, our identification strategies allow us to evaluate the causal effect of bank interventions on firm innovation. Finally, our analysis allows us to evaluate the economic value implications of bank interventions though the innovation channel.

Our paper is also related to a contemporaneous paper, Chava, Nanda, and Xiao (2015), that studies the relation between bank financing and innovation. While Chava, Nanda, and Xiao (2015) also examine the relation between bank interventions and corporate innovation, we push the line of inquiry further by distinguishing two competing hypotheses that are based on different theories. We show, for the first time in the literature, that bank interventions mitigate managerial agency problems and help firms refocus their innovation scope, which, ultimately, enhances firm value. We also document that human capital redeployment is a plausible underlying mechanism.

3. Sample selection, variable construction, and summary statistics

3.1. Data and sample construction

[6] Other studies examine the effects of venture capital investment, product market competition, bankruptcy and labor laws, financial market development, hedge fund activism, firm boundaries, and investors' attitudes toward failure on firm innovation (for example, Kortum and Lerner 2000; Aghion et al. 2005; Acharya and Subramanian 2009; Acharya et al. 2013; Hsu et al. 2014; Seru 2014; Tian and Wang 2014; Brav et al., forthcoming).

We construct two samples on covenant violations. The first sample contains the data used in Nini, Smith, and Sufi (2012) and is obtained from http://faculty.chicagobooth.edu/amir.sufi/. Nini, Smith, and Sufi (2012) collect information on firms' financial covenant violations based on 10-Q or 10-K SEC filings. Their sample includes 10,537 non-financial U.S. firms and 262,673 firm-quarter observations from 1996 through 2008.[7] This data set is used in our baseline OLS analyses. To calculate control variables, we obtain firms' accounting information from the Compustat database and institutional holding data from Thomson's CDA/Spectrum database (form 13F). We end up with a final sample of 8,931 firms and 53,758 firm-year observations. In our sample, 2,400 (26.9%) firms are in violation of financial covenants at least once during the sample period. This observation is similar to that documented in previous studies, which suggests that covenant violations are a fairly common phenomenon (Robert and Sufi 2009; Nini, Smith, and Sufi 2009, 2012).

The second sample is used with the RDD approach (hereafter the RDD sample) to address identification issues. To construct this data set, we limit our attention to a sample of bank loans for which we know the covenant thresholds, as well as any changes (or "buildup") in those thresholds between 1996 and 2008. This analysis alleviates two potential concerns using covenant violations reported in 10-K filings: (1) researchers do not know the exact covenant threshold, and (2) researchers only observe reported covenant violations. We follow Chava and Roberts (2008) and restrict the sample to observations that satisfy the following requirements: (1) they must be non-financial firms that exist in both merged CRSP-Compustat database and the Dealscan database; and (2) they must be firms that have had a loan contract containing either a current ratio or net worth covenant to ensure an accurate measurement of the relevant accounting variable.[8] Current ratio and net worth information is available on a quarterly basis, thus we are able to identify whether a firm is in breach of current ratio or net worth covenants every quarter. However, innovation input and output is measured on an annual basis, so our analysis is conducted on annual frequency. Our final sample consists of all firm-year observations in which a covenant restricting a firm's current ratio or net worth is imposed by a private loan contract recorded in Dealscan

[7] The sample begins in 1996 because 1996 is the first year in which electronic filing became mandatory for all SEC-registered firms, and covenant violations are disclosed in the 10-Q or 10-K SEC filings. Detailed sample selection is provided in Nini, Smith and Sufi (2012).

[8] Covenants restricting the debt to EBITDA ratio, for example, create a problem when trying to measure this ratio with Compustat accounting data since "debt" can refer to any component of a firm's debt structure including: long-term, short-term, senior, junior, secured, total, funded, and so on.

between 1996 and 2008.[9] Our final RDD sample contains 6,280 firm-year observations from 1,642 firms. Among them there are 26% firm-year observations in breach of at least one debt covenant.[10]

We obtain patent and citation information from three sources. First, we retrieve the patent and citation data from the latest version of the NBER Patent Citation database. The NBER database provides information for all utility patents granted by the US Patent and Trademark Office (USPTO) over the period of 1976-2006. Second, we supplement the information for patents granted over the period of 2007-2010, which is provided by Kogan et al. (2017) available at https://iu.box.com/patents. Third, we supplement patent citation information over the period of 2007-2010 using the Harvard Business School (HBS) patent and inventor database that is available at http://dvn.iq.harvard.edu/dvn/dv/patent.

3.2. Variable construction

3.2.1. Measuring innovation

We use two measures to gauge a firm's innovation output. The first measure is the total number of patents applied in a given year (and eventually granted). Following Hall, Jaffe, and Trajtenberg (2001), we use the application year instead of grant year because the actual timing of the patented innovation is closer to the application year. The number of patents captures the quantity of innovation. To measure the quality of innovation output, we construct the second measure, the total number of citations each patent receives in subsequent years. Patenting, captures a firm's innovation activities better than R&D expenditures because patenting is an innovation output variable, which encompasses the successful usage of all (both observable and unobservable) innovation input. In contrast, R&D expenditures only capture one particular observable innovation input (Aghion, Van Reenen, and Zingales 2013) and are sensitive to accounting norms, such as whether they should be capitalized or expensed (Acharya and Subramanian 2009). In addition, more than 50% of firms do not report R&D expenditures in their financial statements in the Compustat database. Koh and Reeb (2015) document that 10.5% of firms with missing values in R&D expenditures in Compustat indeed generate innovation output – patents. Replacing missing values of R&D expenditures with zeros, although a common practice in the existing literature,

[9] Our sample is larger than that in Chava and Roberts (2008) because they restrict their attention to the subsample of firms that experience at least one covenant violation. In contrast, we include the entire sample of firms, including those that have not had any covenant violation in our sample period.

[10] Chava and Roberts (2008) find that 15% and 14% of the firm-quarter observations are in violation of the current ratio and net worth covenants, respectively.

introduces additional noise that could bias the estimated effect of bank interventions on innovation measured by R&D expenditures. Nevertheless patent data are highly skewed, since many firms have no patent in a particular year. Therefore we winsorize R&D expenditures and the patent variables, and take logarithm of patent counts and citations counts to mitigate the skewness concern.

Both innovation measures are subject to truncation problems. Since we only observe granted patents, patents applied in the last several years of our sample may not be granted. Similarly, patents tend to receive citations over a long period, but we observe at best the citations received up to 2010. To deal with these truncation problems, we adjust the patent and citation data by using the "weight factors" computed from the empirical distributions of application-grant lag and by estimating the shape of the citation-lag distribution, respectively.[11] To correct the truncation problem with the number of citations for our extended sample period, we move the adjustment factors created by NBER patent data project forward by four years given that our sample is extended by four years from 2006.[12] Moreover, Hall, Jaffe, and Trajtenberg (2001) suggest that most patents are granted within two years, therefore, we exclude the last two years of patent data (2009-2010) to mitigate the truncation problem.

We merge the patent data with Compustat. Following the innovation literature, we set the patent and citation counts to zero for firm-year observations that are not matched to the patent database, because our patent sample covers the entire universe of publicly-traded firms that have filed patents with the USPTO. The distribution of patent grants in our final sample is right skewed, with its median at zero. Hence, we winsorize these variables at the 99th percentiles and then use the natural logarithm of one plus patent counts (*LnPat*) and the natural logarithm of one plus the number of citations per patent (*LnCite*) as the innovation output measures in our analysis.

One issue regarding using patent counts to capture a firm's innovation output has to do with the timing of patent application and R&D expenditures. While there is significant variation in the time interval between R&D expenditures and patent application across different industries, the average lag is relatively short (for example, typically less than one year). As a result, the existing economics literature (for example, Hall, Griliches, and Hausman 1986; Hausman, Hall,

[11] In particular, we correct the truncation problem of patent counts during the last 6 sample years following Fang, Tian, and Tice (2014).
[12] For example, in the original NBER data, for a patent granted in year 1998 and have a "chemical" classification, the adjustment factor for its citations received is 1.9238. Now, for a patent granted in year 2002 (1998+4) and have a "chemical" classification, the adjustment factor for its citations received is 1.9238.

and Griliches 1984; Lerner and Wulf 2007, and so on.) argues that patent applications are generated nearly contemporaneously with R&D expenditures and uses one-year ahead patent applications to capture innovation output. Following the existing literature, we use one-year ahead patent counts and future citations received by these patents as our main dependent variables. To reflect the long-term nature of investment in innovation, we also measure both innovation proxies in two and three years in the future.

3.2.2. Control variables

Following the prior literature in innovation, we control for a set of firm and industry characteristics that might affect a firm's future innovation output. All variables are computed for firm *i* over its fiscal year *t*. In the baseline OLS regressions, the control variables include firm size, *Ln(AT)*, measured by the natural logarithm of total assets; *PPE_Assets*, measured by net property, plant and equipment divided by total assets; *CAPEX_Assets*, measured by capital expenditures divided by total assets; product market competition, *HI*, measured by the Herfindahl index based on annual sales; institutional holdings, *INST*, calculated as the arithmetic mean of the four quarterly institutional holdings reported through form 13F; *Z-Score*, calculated based on Altman's (Altman 1968) formula. To circumvent potential non-linear effects of product market competition (Aghion et al. 2005), we include the squared Herfindahl index in our baseline regressions.

In addition, we follow Sufi (2009) and Nini, Smith, and Sufi (2012) and include five covenant variables: *Debt_Assets*, the ratio of book value of total debt to total assets; *ROA*, the ratio of operating cash flow to total assets; *Net worth-to-assets*, net worth (total assets minus total liabilities) to total assets ratio; *Current ratio*, the ratio of current assets to current liabilities; *Interest-to-assets*, interest expenses to total assets ratio. These variables represent the most common ratios used in financial covenants and thereby are strong predictors of debt covenant violations (Roberts and Sufi 2009). We also control for Tobin's *Q*. We describe detailed variable definitions in the Appendix A. Since the control variables are potentially endogenous, we estimate baseline regressions without controls and find that the results are robust to doing so.

3.3. Summary statistics

To mitigate the effect of outliers, we winsorize all control variables at the 1st and 99th percentile of their distribution. Panel A of Table 1 presents summary statistics for our first covenant violation sample during 1996 to 2008 obtained from Amir Sufi's debt covenant violations dataset. On average, a firm in our sample generates 3.43 patents per year and each patent receives 2.99 subsequent citations. In our sample, about 6% of firm-year observations are in violation of financial covenants, suggesting that covenant violations are not a rare event. An average firm in our sample has a total asset of $99 million, *PPE_Assets* ratio of 0.26, *CAPEX_Assets* ratio of 0.06, institutional holdings of 26%, and a *Z-Score* of 1.13.

Panel B of Table 1 presents summary statistics for the RDD sample containing all firm-year observations in which a covenant restricting the current ratio or net worth of the firm is imposed by a private loan contract included in Dealscan during 1996 to 2008. A firm on average produces 2.08 patents per year and each patent receives 2.35 subsequent citations. In our sample, about 26% of firm-year observations are in violation of at least one of the current ratio and net worth covenants. On average our sample firm has a total asset of $244 million, *PPE_Assets* ratio of 0.32, *CAPEX_Assets* ratio of 0.07, institutional holdings of 33%, and a *Z-Score* of 1.36. Table 2 provides univariate comparisons of firms with high R&D and low R&D expenditures using the RDD sample. Because the median of R&D expenditures in our sample is zero, we define high R&D-intensive firms as those with positive R&D expenditures and low R&D-intensive firms as those with zero R&D expenditures. We report the mean values of firm characteristics of high R&D-intensive and low R&D-intensive firms in columns (1) and (2), respectively. The differences in mean values between the two groups of firms are reported in column (3). Since observations are not independent over time for each firm, we report in column (4) P-values based on standard errors clustered by firm to avoid overstating the degrees of freedom. High R&D-intensive firms use significantly less debt in their capital structure than low R&D-intensive firms, which is consistent with early findings of a negative relation between firm leverage and R&D expenditures, documented in the prior literature (see Hall and Lerner (2010) for a survey of this literature). Interestingly, we find that the frequency of violating debt covenants is 23% in high R&D-intensive firms, which is lower than that in low R&D-intensive firms (i.e., 28%). This observation could be due to the fact that high R&D-intensive firms have a much lower level of debt compared to low R&D-intensive firms, leading to much lower likelihood of violating debt covenants. In addition, compared to low R&D-intensive firms, high R&D-intensive firms exhibit greater innovation output, a lower level of total assets, lower *CAPEX*, lower *ROA*, but higher *Tobin's Q.* All these observations are consistent with earlier finding that high R&D-intensive firms are those with greater growth potential.

4. Empirical results

4.1. Baseline OLS results

We first provide summary statistics that compare the innovation variables across violating and non-violating firms in year *t+1* in Table 3. We report the mean values of innovation variables in violating and non-violating firms between 1996 and 2008 in columns (1) and (2), respectively. In columns (3) and (4), we present the mean difference between violators and non-violators, and the corresponding P-value testing the null hypothesis that the differences are zero. Based on the univariate t-tests, violating firms have significantly lower patent counts, citation counts per patent, and R&D to assets ratio.

Next, we assess the effect of bank interventions on firm innovation with the following OLS regressions:

$$(InnovationVariable)_{i,t+n} = \alpha + \beta Violation_{i,t} + \gamma' Controls_{i,t} + Year_t + Firm_i + \varepsilon_{i,t} \quad (1)$$

where i indexes firm, t indexes time, and n equals one, two, or three. The dependent variables are $LnPat_{i,t+n}$ and $LnCite_{i,t+n}$, where $LnPat_{i,t+n}$ is the natural logarithm of one plus the total number of patents filed (and eventually granted) in one, two, and three years, and $LnCite_{i,t+n}$ is the natural logarithm of one plus the number of future citations received per patent for patents filed in one, two, and three years. The variable of interest, $Violation_{i,t}$, is a dummy that equals one if a covenant violation occurs in year t for firm i and is not preceded by a violation in the previous four quarters, and zero otherwise. Following existing literature, we include a vector of control variables that may affect a firm's innovation output as we discussed in Section 3.2.2. We include year fixed effects, $Year_t$, and firm fixed effects, $Firm_i$, in all regressions.

We report the results estimating equation (1) in Table 4. For brevity we report the results only for year t+1. We start with a parsimonious model without any control variables since these controls are potentially endogenous. The coefficient estimates of *Violation* are negative and significant in column (1) in which the dependent variable is the number of patents in year t+1.[13] This evidence suggests that a covenant violation is associated with a significant reduction in patent

[13] In an unreported test, we find that *Violation* is negatively related to patent counts in year *t+2*.

counts following the violation. In column (2), we replace the dependent variable with patent quality in year t+1 and find that the coefficient estimate on *Violation* is negative but not statistically significant. We find a similar result in years t+2 and t+3.[14] Next, we add a set of controls that are important determinants of firm innovation, *Controls,* found by existing studies in the regressions. For brevity we suppress the coefficient estimates of control variables. We continue to observe a significant drop in patent counts but not patent citations in the years following covenant violations.[15] A word of caution is that, while the patent quality results are statistically insignificant, we observe that the number of citations per patent decreases after covenant violations in columns (2) and (4) and the decline is typically of the same order of magnitude as patent counts. Thus, it is possible that the effect on citations is much noisier and therefore we cannot reject the null hypothesis that patent quality does not change after bank interventions.

For completeness, we examine the effect of covenant violations on an innovation input – R&D expenditures, and report the results in the Online Appendix. We find that bank interventions do not appear to affect *R&D/Assets* in any of the three subsequent years following the violation.[16] Patenting, however, captures a firm's innovation activities better than R&D expenditures because patenting is an innovation output variable, which encompasses the successful usage of all (both observable and unobservable) innovation input. In contrast, R&D expenditures only capture one particular observable innovation input (Aghion, Van Reenen, and Zingales 2013) and are sensitive to accounting norms, such as whether they should be capitalized or expensed (Acharya and Subramanian 2009). In addition, more than 50% of firms do not report R&D expenditures in their financial statements in the Compustat database. Koh and Reeb (2015) document that 10.5% of

[14] Regarding why we find a significantly negative effect of covenant violations on the quantity of patents whereas Chava et al. (2015) show no impact of covenant violations on patent quantity, a plausible reason is that we include more control variables than they do in the analysis. We follow the existing literature, for example, He and Tian (2013) and Chemmanur et al. (2014), when choosing control variables. To better understand the difference between our results and theirs, we have tried to replicate their results by estimating the regressions with the same set of control variables as reported in Table 9 of Chava et al. (2015). We find significant reduction in the level of *Ln(R&D)* in years 1 and 2 after the violation but no significant reduction in the level of *Ln(Pat)* and *Ln(Cite)* in regressions with firm fixed effects. Hence, the difference between our results and theirs on the effect of covenant violations on patent quantity is due to the inclusion of control variables.

[15] To save space, we suppress the coefficient estimates of these control variables in this test as well as the following tests. They, however, are available upon requests.

[16] Chava et al. (2015) find a significant drop in R&D growth in year 1 and year 2 after the violation. There are a few differences between our analyses and those of Chava et al. (2015). First, we conduct all analyses using annual data, whereas Chava et al. (2015) use quarterly data. Second, our dependent variable is *R&D/Assets,* with firm fixed effects we examine the first difference in *R&D/Assets*. However, in Chava et al. (2015), their dependent variable is the "cumulative" change of R&D over time since the violation year, namely, the growth rate of R&D in year $t+2$ is computed as $\Delta Ln(R\&D)_{t,t+8}$ where t represents quarter. As a result, $\Delta Ln(R\&D)_{t,t+8}$ is the change in R&D level from year t to year $t+2$, which includes a change over a two-year window. We are able to replicate their results, where we find a significant drop in cumulative R&D growth in both year 1 and year 2. Our coefficient estimates on *Violation* are similar to those in Table 2 of Chava et al. (2015).

firms with missing values in R&D expenditures in Compustat indeed generate innovation output – patents. Replacing missing values of R&D expenditures with zeros, although a common practice in the existing literature, introduces additional noise that could bias the estimated effect of bank interventions on innovation measured by R&D expenditures.

We undertake various robustness tests for our baseline specifications. To save space, we tabulate these robustness test results in the Online Appendix. First, we employ an alternative econometric model, the quantile regression model, because of the skewness of patent and citation counts.[17] We find that the coefficient estimates of *Violation* are negative and significant in all specifications for patent counts when we run the quantile regressions at the 80th percentile. The coefficient estimates of *Violation*, however, are insignificant in all the regressions explaining patent quality. The results are similar if we run the quantile regressions at other percentiles.

Second, we check the robustness of our results using alternative proxies for innovation quality, patent originality and generality that capture the fundamental importance of innovation, following Hall, Jaffe, and Trajtenberg (2001). Patents that cite a wider array of technology classes of patents are considered as having greater originality. In a similar spirit, patents that are being cited by a wider array of technology classes of patents are viewed as having greater generality. We find that *Violation* is generally not significantly related to either patent generality or patent originality. The results are consistent with our baseline findings of statistically insignificant change in citation counts after covenant violations. Third, a reasonable concern is that large firms often enhance innovation by acquiring small firms (Sevilir et al. 2016). In the meantime, covenant violating firms make substantially less acquisitions (Nini, Smith, and Sufi 2012). Therefore, our baseline findings may be affected by firms' acquisitions. To address this concern, we construct a variable, *AcqAssets*, which equals a firm's acquisition expenditures normalized by its total assets, and include it in equation (1). We obtain both quantitatively and qualitatively similar results.

4.2. Innovation dynamics

While our baseline results suggest that there is a negative relation between covenant violations and patent counts, the results may be driven by reverse causality. In other words, reductions in innovation activities associated with poor investment opportunities lead firms to

[17] We include year fixed and industry fixed effects in the quantile regressions because a non-linear model such as quantile regressions with firm fixed effects does not converge.

violate debt covenants. To address this concern and understand the dynamics of innovation output surrounding the violation, in Table 5, we present the regression results that estimate the following model:

$$InnovationVariable_{i,t+1} = \alpha + \beta_1 Violation_{i,t+2} + \beta_2 Violation_{i,t+1} + \beta_3 Violation_{i,t} + \beta_4 Violation_{i,t-1,t-2} + Year_t + Firm_i + \varepsilon_{i,t}. \quad (2)$$

where i indexes firm and t indexes time. The dependent variables, $LnPat_{t+1}$ and $LnCite_{t+1}$, are the natural logarithm of one plus total number of patents and the natural logarithm of one plus the number of citations received per patent, respectively, in year $t+1$. $Violation_{t+2}$, $Violation_{t+1}$, and $Violation_t$ are dummy variables that equal one if a covenant violation occurs in year $t+2$, $t+1$, and t, respectively, and zero otherwise. $Violation_{t-1,\, t-2}$ is a dummy variable that equals one if a covenant violation occurs in year $t-1$ or year $t-2$ and zero otherwise.

We find statistically insignificant coefficient estimates of β_1 and β_2 in all models, which suggests that there is not a pre-existing trend in firm innovation output. In column (1), we find significantly negative coefficient estimates of both β_3 and β_4, implying a significant reduction in patent counts in the first three years following the violation. Once again, we cannot reject the null hypothesis with respect to the effect of bank interventions on innovation quality in column (2). Overall, the results on innovation dynamics suggest that our main findings are not driven by reverse causality.

4.3. The regression discontinuity design

In this section, we further address endogeneity concerns caused by omitted variables and reverse causality. Our identification strategy is to use the RDD, following Chava and Roberts (2008). This approach relies on "locally" exogenous variation in covenant violations generated by the distance to the covenant threshold. It essentially compares the innovation variables of firms that just violate covenants to those that barely avoid violating covenants. The RDD is a powerful identification strategy because for these firms falling in a narrow band of the distance to the covenant threshold, the violation is close to an independent, random event and therefore is unlikely correlated with firm unobservable characteristics.

For this purpose, we limit our attention to a sample of bank loans for which we know the covenant thresholds, as well as any changes (or "buildup") in those thresholds over time during 1996-2008. Our final sample consists of all firm-year observations in which a covenant restricting a firm's current ratio or net worth is imposed by a private loan contract recorded in Dealscan, which is the RDD sample discussed in more detail in Section 3.1.

Following the existing literature (namely, Lee and Lemieux 2010; Cuñat, Gine, and Guadalupe 2012; Bradley et al. 2017), we start our RDD analysis with an estimation of a polynomial model that makes use of all the observations in the sample. This method allows us to incorporate the precise distance to the covenant threshold into our regression specification. Specifically, we estimate the following model:

$$\begin{aligned} &InnovationVariable_{i,t+n} = \alpha + \beta Violation_{i,t} + P_l(CR_{i,t}) + P_r(CR_{i,t}) \\ &+ P_l(NW_{i,t}) + P_r(NW_{i,t}) + Controls_{i,t} + Year_t + Firm_i + \varepsilon_{i,t}, \end{aligned} \tag{3}$$

where *i* denotes firm, *t* denotes time, and *n* equals one, two, or three. To determine whether or not a firm is in violation, we compare the firm's actual accounting measure (namely, current ratio or net worth) to the covenant threshold implied by the terms of the debt contract. $Violation_{i,t}$ is a dummy variable that equals one if a firm's current ratio or net worth falls below the corresponding covenant threshold in any of the four quarters in a fiscal year. $P_l(CR_{i,t})$ and $P_r(CR_{i,t})$ are flexible polynomial functions of the distance to the threshold on the left-hand and right-hand side, respectively, with respect to the current ratio covenant threshold for firm *i* with different orders. $P_l(NW_{i,t})$ and $P_r(NW_{i,t})$ are flexible polynomial functions of the distance to the threshold on the left-hand and right-hand side, respectively, with respect to the net worth covenant threshold for firm *i* with different orders. Distance to the threshold is the absolute difference between current ratio (net worth scaled by total assets) and the corresponding covenant thresholds (thresholds scaled by total assets).[18] *Controls* includes the same set of control variables as those in the OLS models.

A potential concern of the RDD analysis is that firms may engage in manipulation of their accounting statements to avoid violating a debt covenant. To address this concern, we do two things. First, we examine the distribution of firms around the covenant threshold. We perform a formal statistic test, developed by McCrary (2008), to check the discontinuity in the density of the forcing variable (current ratio or net worth), and report the results in Figure 1. Panels A and B report the results with current ratio and net worth covenants, respectively. The diamonds represent

[18] If a firm does not violate covenants, we include in the regressions the polynomials of the minimum distance to the threshold in all four quarters. If a firm violates covenant in a particular quarter, we use the polynomials of the distance to the threshold in the violating quarter. If, however, a firm violates covenant in more than one quarter in a fiscal year, we use the polynomials of the minimum distance to the threshold.

the density estimates and the bold line is the fitted density function of the forcing variable surrounded by the 95% confidence interval. As one can observe, the density of the distance to covenant threshold appears smooth and its fitted curves show little indication of strong discontinuity near the threshold. The *Z*-statistic for the McCrary test of discontinuity is 0.400 and 0.885 for the current ratio and net worth sample, respectively, which is statistically insignificant. Thus we are unable to reject the null hypothesis that the density function at the threshold is continuous, suggesting no evidence that firms have been able to precisely manipulate the earnings around the known threshold. Our result is consistent with the findings in Falato and Liang (2016). Second, to control any remaining effect of earnings management on our results, following Chava and Roberts (2008) and Roberts and Sufi (2009), we include into our regressions two measures of abnormal accruals (Dechow and Dichev 2002; Teoh, Welch, and Wong 1998). We present detailed definitions of these two accrual variables in Appendix A.

The key variable of interest is β, which captures the causal effect of bank interventions on firm innovation output n years after the covenant violation. β is identified under the assumption that managerial preferences over innovation investment are not discontinuous exactly at the covenant threshold. Note, however, that due to the local exogeneity nature of the RDD, this coefficient estimate should be interpreted locally in the immediate vicinity of the covenant violation threshold.

We present the results estimating equation (3) using the full sample in Table 6 Panel A. We report the result with polynomials of order two, but our results are qualitatively similar using other polynomial orders. For brevity, we report the results only for year t+1. The coefficient estimates of *Violation* are significant in column (1), suggesting a reduction in patent counts in the first year following the covenant breach. Consistent with our earlier findings, the coefficient estimates of *Violation* are insignificant in column (2), suggesting that we cannot reject the null hypothesis that patent quality does not change after bank interventions.[19] Once again, the results remain robust when we include all control variables in the regressions, as shown in columns (3) and (4).

We also conduct the RDD in alternative forms by considering a narrow "band" around the covenant threshold. Thus, these firms' current ratio or net worth falls either right above or right

[19] Unreported results show that patent counts drop significant in the second year follow covenant violation. Patent quality, however, does not change significantly in any of the three years post-violation.

below the covenant threshold. Following Chava and Roberts (2008), we restrict the sample to those observations in which the absolute value of the distance to the covenant threshold scaled by the threshold is shorter than 0.20. We report the results in the Online Appendix. We find generally similar results that covenant violations affect a firm's innovation quantity but have no statistically significant effect on a firm's innovation quality.[20]

For completeness, we once again examine the effect of covenant violations on R&D expenditures in the RDD framework. As shown in the Online Appendix, there is no significant change in *R&D/Assets* in any of the three years after covenant violations using the entire sample. As we restrict our sample to a narrow "band" around the covenant threshold, the coefficient estimates of *Violation* are largely insignificant. Overall, we find little evidence that covenant violations affect a firm's R&D spending.

Since many firms have zero R&D expenditures or never had a patent, one concern is that whether our results are driven by a contrast between firms that do or do not have R&D or patents. To address this concern, we re-run the regressions in Table 6 using a subsample of firms that excludes those that had never reported any R&D or had never had any patents during our sample period. The results are reported in Panel B and C of Table 6. In Panel B, we exclude the firms that had never reported any R&D during our sample period, and so the sample is reduced from 7,288 to 2,753 observations. In Panel C, we exclude the firms that had never had any patents during our sample period, and thus the sample is reduced to 2,496 observations. Although the sample becomes much smaller, our results remain robust. There is a significant decline in patent counts after covenant violations. Patent citations decrease yet statistically insignificant after the violations. In fact, the effects in the subsample are considerably larger than for the full sample, suggesting that our findings are not simply about a contrast between firms that do or do not innovate.

In summary, our results obtained from the OLS and the RDD analyses suggest that there appears a negative, causal effect of bank interventions on firm innovation quantity. However, bank interventions do not appear to affect innovation input or innovation quality.

4.4. Cross-sectional variation in the innovation response

[20] Since R&D is expensed, firms could reduce their R&D expenditure even before the occurrence of debt covenant violations. If this is indeed true, our results are more conservative than the real effects of bank interventions.

In this subsection, we make a first attempt to understand the underlying reasons why bank interventions reduce firm innovation quantity by distinguishing the value-destroying hypothesis and the value-enhancing hypothesis. According to the arguments of Jensen (1986), Aghion and Bolton (1992), and Dewatripont and Tirole (1994), creditor interventions could enhance firm value by mitigating value-destroying managerial actions, such as excessive investment in innovation projects, which arises from conflicts of interest between managers and shareholders. If the reduction in patent counts upon covenant violations is a result of the shift in control rights from shareholders to creditors thus alleviating the agency problems, namely, the value-enhancing hypothesis is supported, we expect firms with more severe ex ante agency problems to be affected more by this control right shift. To explore this conjecture, we divide our sample firms into two groups: those with high vs. low agency problem. Following Chava and Roberts (2008), we use the following model specification in the RDD framework:

$$\begin{aligned} LnPat_{i,t+n} &= \alpha + \Gamma_0 \mathrm{I}_{(\omega)} \mathrm{X}_{\mathrm{it}} + \Gamma_1 (1 - \mathrm{I}_{(\omega)}) \mathrm{X}_{\mathrm{it}} + P_l(CR_{i,t}) + P_r(CR_{i,t}) + P_l(NW_{i,t}) \\ &+ P_r(NW_{i,t}) + Year_t + Firm_i + \varepsilon_{i,t} \end{aligned}, \qquad (4)$$

where $I_{(\omega)}$ is an indicator function equal to one if ω is true and zero otherwise, X is a vector of variables including a violation indicator and all control variables as described in Section 4.3. $P_l\ (CR_{i,t})$ and $P_r\ (CR_{i,t})$ are flexible polynomial functions (order of two) of the distance to threshold on the left-hand and right-hand side, respectively, with respect to the current ratio covenant threshold for firm *i*. $P_l\ (NW_{i,t})$ and $P_r\ (NW_{i,t})$ are flexible polynomial functions (order of two) of the distance to the threshold on the left-hand and right-hand side, respectively, with respect to the net worth covenant threshold for firm *i*. The dependent variable, $LnPat_{i,t+n}$, is the natural logarithm of one plus total number of patents filed (and eventually granted) in one, two, and three years.

We consider three proxy variables that capture the degree of agency problems: (1) credit ratings. We construct *Has (no) rating*, a dummy variable that takes a value of 1 if the firm has either (neither) a bond rating or (nor) a commercial paper rating, and zero otherwise; (2) prior lending relationship. We construct *Has (no) relationship,* a dummy that is equal to 1 if the lead bank of the current loan has (never) acted as a lead bank for any loan from the borrowing firm during the previous 5 years, and zero otherwise; (3) loan syndication size. We construct *Large syndicate size*, a dummy that equals 1 if the lead bank syndicate consists of 5 or more lenders, and zero otherwise, and a *Small syndicate size* dummy that equals 1 if the lead bank syndicate consists of less than 5 lenders. We expect that firms without credit ratings, without prior lending relationship, or with small syndicate size are subject to more severe agency problem, because these firms are subject to a lower degree of monitoring by the market or lenders.

We report the results estimating equation (4) in Table 7. We include in the RDD regressions the interaction terms of these dummy variables with the *Violation* indicator to allow the impact of covenant violations to vary among firms with high vs. low level of agency problems. In columns (1), (2), and (3), we observe that bank intervention causes a significant decline in patent counts one year post-violation only in firms that are subject to more severe agency problems, namely, those firms that have no credit rating, have no prior lending relationship, or have a small syndicate size. We find, in unreported analyses, some consistent evidence for the second year after violations, too.[21] These observations suggest that banks have a greater impact to streamline innovation projects in firms that face more severe agency conflicts and hence are more likely to invest in low quality patents.

5. Economic value implications of bank interventions

Our results so far suggest that bank interventions triggered by covenant violations cause a significant reduction in a firm's patent quantity but not patent quality. As a result, it is unclear if the reduction in a firm's innovation quantity is value-enhancing or value-destroying. Existing literature tends to find that innovation output is positively associated with firm value (Hall, Jaffe, and Trajtenberg 2005), and hence our findings could imply that creditors reduce firm value by impeding innovation. On the other hand, theories (for example, Jensen 1986; Aghion and Bolton 1992; Dewatripont and Tirole 1994) argue that creditor interventions might enhance firm value by mitigating value-destroying managerial actions, such as excessive investment in innovation projects, which arises from conflicts of interest between managers and shareholders. For example, Scharfstein and Stein (2000) argue that specialized investment, such as investment in innovation projects, effectively entrenches the management. In addition, overconfident managers or managers with career concerns who want to "grandstand" could overinvest in innovation that may not necessarily best serve shareholders' interest (Hirshleifer, Low, and Teoh 2012; Gompers 1996). Therefore, our findings could also imply that bank interventions cut firm investment in "bad" innovation projects and hence enhance firm value.

[21] Unreported tests show that the results in Table 7 remain robust when we leave out control variables or use a subsample of firms that excludes those that have never reported any R&D or have never had any patents during our sample period.

In this section, to distinguish the value-destroying hypothesis and the value-enhancing hypothesis, we focus on the "bottom-line" question regarding the economic consequences of innovation reductions after bank interventions. Specifically, we examine how a reduction in innovation output affects firm value and explore plausible channels through which this occurs.

5.1. Economic value effect

To investigate the value implications of our findings that bank interventions reduce innovation output, we focus on the economic value of new innovation that is based on stock market reactions to patent grants (Kogan et al. 2017). The advantage of using stock market reactions to capture patent value is that asset prices are forward-looking and hence provide us with an estimate of the private value to the patent holder that is based on ex-ante information.[22]

In Table 8, we investigate how violations of debt covenants affect the average value of patents produced by firms in the RDD framework. In addition to the standard control variables used in Table 6, we follow Kogan et al. (2017) and include an additional control – *Idiosyncratic Volatility*, which is the standard deviation of the difference between monthly return of stock and market return over the fiscal year. We include year fixed effects and firm fixed effects in the regression, where the dependent variable is *Average Patent Value*$_{i,t+1\text{->}t+3}$ that is the average value of all patents applied within a three-years window subsequent to a given sample year, respectively. The coefficient estimates of the violation dummy are positive and significant at the 5% level, suggesting that a significant increase in the average value of patents applied during the three years post-violation, compared to the value of those filed before the violation. The economic effect is sizable: the average value of patents applied during three years post-violation is $0.462 million larger than that of patents applied prior to the covenant violation. Since the mean patent value of pre-violation firms is $0.66 million, this is equivalent to a 70% increase in patent value.[23] Hence, the evidence suggests that, although violating firms file fewer patents after interventions, these patents are better received by the market, which increases firm value.

To understand why a reduction in innovation output after bank interventions is associated with an increase in firm value, we postulate that changes in covenant violating firms' innovation

[22] The market value of new patent grants is computed by Kogan et al. (2017) and available at https://iu.box.com/patents. The dollar values of patents are deflated by 2005 dollars using the Consumer Price Index (CPI) from the Bureau of Labor Statistics.

[23] The results are robust when we drop all control variables or use a subsample of firms that excludes those that have never reported any R&D or have never had any patents during our sample period.

scope is a plausible reason. To the extent that a focused innovation scope allows firms to make the best use of their limited physical resources and human capital, which enhances firm value, we postulate that bank interventions help firms adjust their innovation scope and push them to cut tangent patents that are unrelated to their core business.

Innovation activities that are unrelated to a firm's core business are likely to be out of managers' expertise and hence are likely value-destroying. Managers who pursue such innovation activities are probably for their own private benefits rather than enhancing firm value. For example, Hirshleifer, Low, and Teoh (2012) show that overconfident CEOs tend to invest more in innovation even if their firms are not in innovative industries and such an investment does not contribute to firm value. Bank interventions upon covenant violations mitigate misaligned incentives between managers and shareholders and thus curtail investment in such innovation activities. In contrast, creditors preserve innovation activities that are related to the firm's core business, which should enhance firm value.

We test this conjecture by first classifying a firm's patents into two categories: patents that are related to the firm's core business (labeled as related patents) and patents that are unrelated to the firm's core business (labeled as unrelated patents). Specifically, we define patents that are in a firm's main 2-digit SIC industry as related patents, and patents that are not in a firm's main two-digit SIC industry as unrelated patents. A practical difficulty, however, is that the USPTO does not assign a patent's industry membership in the SIC framework. Instead, the USPTO adopts a patent classification system that assigns patents to 3-digit technology classes that are based on technology categorization instead of final product categorization.[24] We use a concordance table that connects the USPTO technology classes to 2-digit SIC codes developed in Hsu, Tian, and Xu (2014) to map patents in each technology class to one or multiple 2-digit SIC codes. We then compute the number of related patents in a firm's main 2-digit SIC industry by multiplying patent counts with the corresponding mapping weights. We calculate the number of unrelated patents by subtracting the number of related patents from the total number of patents a firm has in a year.[25]

[24] The details of technology classes can be found at http://www.uspto.gov/web/offices/ac/ido/oeip/taf/cbcby.htm.

[25] For example, 63% of USPTO technology class 1 is mapped to two-digit SIC industry 35, 32% of technology class 1 is mapped to two-digit SIC industry 36, and 5% of technology class 1 is mapped to two-digit SIC industry 37. USPTO technology class 7 is mapped to ten two-digit SIC industries, with 13% of patents mapped to two-digit SIC industry 35. Suppose that a firm's main two-digit SIC code is 35, and it has 3 and 5 patents in USPTO technology class 1 and 7, respectively. Then the number of patents that is related to this firm's core business is calculated as 3*63%+5*13% = 2.54, and the number of patents that are not in its core business is 5.46 (= 3 + 5 – 2.54).

Next, we investigate the change in patent counts in each of these two groups of patents surrounding covenant violations in the RDD framework. We report the results in Table 9. The dependent variables in columns (1) and (2) are the total number of unrelated patents and related patents, respectively, generated within three years after violations. In column (1), we find negative and significant coefficient estimate of *Violation*, suggesting that firms exhibit a significant reduction in the number of unrelated patent within three years following bank interventions. In column (2), the coefficient estimate of *Violation* is statistically insignificant, which suggests that bank interventions are not related to significant changes in innovation output related to a firm's core business.[26] The Wald test of the coefficient estimates on *Violation* across columns (1) and (2) suggests a significant difference in the effects of covenant violations on the numbers of unrelated and related patents the violating firm generates.[27] These findings lend support to our conjecture that bank interventions curb firms' excessive investment in innovation projects unrelated to their core business, allowing firms to focus more on innovation within their expertise.[28]

5.2. Linking refocus of innovation scope to firm value

Next, we link our finding on firms refocusing their innovation scope after bank interventions to the firm value analysis reported in Table 8. If our conjecture that core business patents ("related patents") are more value enhancing than peripheral patents ("unrelated patents") is correct, we expect that the abnormal returns at patent grants (namely, economic value of patent) should be higher for related patents. For this purpose, we examine the relation between patents relatedness and average value of patents based on the OLS sample (containing all U.S. and non-financial Compustat firms between 1996 and 2008) and report the results in the Online Appendix. We find that the percentage of patents related to firms' core business within a three-year window is significantly positively related to the average value of patents. The economic effect is sizable:

[26] Unreported results show that the findings in Table 9 remain robust when we leave out control variables or use a subsample of firms that excludes those that have never reported any R&D or have never had any patents during our sample period.

[27] A reasonable concern is that many firms (especially large firms) have multiple business segments that involve several different industries. Hence, defining patent relatedness only based on the firm's main SIC industry membership may bias the results. To address this concern, we obtain each firm's SIC industries listed in the Compustat Business Segment file and treat all of them as parts of the firm's core business, based on which we define related and unrelated patents. Specifically, we define related patent as those that are in any of a firm's main 2-digit SIC industries listed in the Compustat Business Segment file, and unrelated patents as those that are not in any of the firm's main 2-digit SIC industries. We compute the number of related patents in every 2-digit SIC segment industry of a firm by multiplying patent counts with the corresponding mapping weight from the concordance table that connects the USPTO technology classes to 2-digit SIC codes developed in Hsu, Tian, and Xu (2014). We repeat the analyses in Table 9 and report the results in the Internet Appendix. We find similar results.

[28] Our finding is also consistent with Lerner, Sorensen, Stromberg (2011) who show that patent portfolios of LBO target firms become more focused in the years after private equity investment.

A 10 percentage increase in the fraction of related patents is associated with a $0.4 million increase in firm value. Our results suggest that bank interventions enhance firm value mainly through cutting a firm's unrelated patents thus increasing the fraction of related patents. The evidence is consistent with the value-enhancing hypothesis.

5.3. Human capital redeployment

Our findings from Sections 5.1 and 5.2 suggest that covenant violating firms refocus their innovation scopes after bank interventions, which enhances their firm value. In this subsection, we explore a human capital redeployment mechanism through which bank interventions curb excessive innovative projects that are unrelated to a firm's core business. We investigate this human capital redeployment mechanism by exploring individual inventor turnover.

Because individual inventors are key input of innovation, we postulate that bank interventions push firms to refocus on innovative projects within their expertise through layoffs of inventors whose skill sets are unrelated to their core business and hiring inventors who have track records of producing patents related to their core business. We also conjecture that creditors encourage inventors who stay within the firm to develop skills and produce more related patents after their interventions.

To investigate this possible mechanism, we restrict our sample to a window of three years before and three years after the covenant violation for both the violating and non-violating firms. We follow existing studies (for example, Brav et al., forthcoming) and identify three groups of inventors. The first group of inventors is "leavers": the inventors who produce at least one patent in a firm three years before the violation but none after, and at least one patent in a different firm three years after the violation. The second group of inventors is "new hires": the inventors who produce no patent three years before the violation and at least one patent three years after the violation in a firm, and produce at least one patent in a different firm three years before the violation but none in different firms after the violation. The third group of inventors is "stayers": the inventors who produce at least one patent in a firm both three years before and after the violation but produce no patent in any other firms before or after the violation.

If human capital redeployment is a mechanism through which bank interventions curtail excessive innovation unrelated to a firms' core business, we expect to observe that "leavers" are less likely to specialize in areas that are related to the firm's core business and hence generate less related patents than ”stayers” in the violating firms. Meanwhile, when firms recruit new talents, the violators tend to hire inventors who have a better track record of producing patents related to their core business than those they fire, i.e., the leavers. Regarding stayers of the violating firms who generate patents both before and after the covenant violation, we expect them to focus more on projects that are related to the firms' core business and hence generate more related patents after bank interventions compared to the stayers of the non-violating firms. We report the results testing these conjectures in Table 10.

In Panel A, we report the difference-in-differences (DiD) test results that compare the difference in the percentage of related patents produced by stayers and that by leavers in covenant-violating firms with the same difference in non-violating firms. In column (1), we first report the average difference between the percentage of related patents generated by stayers over the three-year period preceding the violation and the percentage of related patents generated by leavers over the same period in covenant-violating firms. In column (2), we repeat the same procedure for non-violating firms and report the average differences in the percentage of related patents between stayers and leavers in column (2). We then report the difference in differences between violators and non-violators in column (3). We report the *p*-values of the two-tailed *t*-statistics testing the null hypothesis that the mean differences are zero in column (4).

We find that the percentage of related patents produced by stayers is significantly higher than that produced by leavers in violating firms, whereas the opposite is observed in non-violating firms. The DiD estimator is negative and is significant at the 1% level. This finding suggests that violating firms actively fire inventors who are not good at producing patents related to their own core business while retain those who are better at producing related patents.

By the same token, we report in Panel B the result of the DiD test comparing the difference in the percentage of related patents filed by new hires and that filed by leavers in covenant-violating firms with the same difference in non-violating firms. The percentage of related patents produced by new hires is significantly higher than that produced by leavers in violating firm. However, new hires produce fewer related patents than leavers in non-violating firms. The DiD estimator is 0.269 and is significant at the 1% level, suggesting that covenant violating firms actively hire new inventors who are better at producing related patents while fire those who are not good at producing related patents.

In Table 11, we report the results for stayers. We first subtract the percentage of related patents generated over the three-year period preceding the violation from the percentage of related patents generated over the three-year period after the violation for each stayer of the treatment (violating) firms. The difference is then averaged over all stayers in the treatment group and reported in column (1). We repeat the same procedure for stayers of the control (non-violating) firms and report the results in column (2). We then calculate the differences between columns (1) and (2) and report the results in column (3). We report the *p*-value of the statistics in column (4). The difference is positive and significant at the 5% level, suggesting that, compared with the stayers of non-violating firms, stayers of the violating firms generate a greater fraction of patents that are related to the firms' core business after bank interventions.

In summary, our evidence reported in this subsection is consistent with the conjecture that upon a covenant violation, creditors help the firm refocus its innovation scope by firing inventors who are not specialized in the firm's core business. Inventors who stay in the firm produce patents that are more related to the firms' core business after the violation. Overall, human capital redeployment appears a plausible underlying mechanism through which creditors help violating firms refocus its innovation scope, which ultimately enhances firm value.

6. Conclusion

In this paper, we examine the effect of bank interventions triggered by debt covenant violations. We show that bank interventions negatively affect innovation quantity but do not affect innovation quality. We further document that the reduction in innovation quantity is concentrated in innovation projects that are unrelated to a firm's core business, which leads to a more focused scope of innovation output and ultimately an increase in firm value. Human capital redeployment appears a plausible mechanism through which bank interventions refocus innovation scope and enhance firm value. Our findings are consistent with the argument that creditors help mitigate investment distortions in innovation arising from conflicts of interest between managers and shareholders and shed new light on the real effect of bank financing.

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Table 1
Summary Statistics

Panel A: OLS Sample

Variable	N	Mean	S.D.	P25	Median	P75
Pat	53,758	3.43	13.91	0.00	0.00	0.00
Cite	53,758	2.99	9.26	0.00	0.00	0.00
Violation dummy	53,758	0.06	0.23	0.00	0.00	0.00
R&D/Assets (per $1000 Assets)	53,758	76.60	183.77	0.00	0.00	68.09
Ln(AT)	53,758	4.60	2.59	2.93	4.66	6.40
PPE_Assets (per $1000 Assets)	53,758	264.79	235.82	76.49	189.36	390.75
CAPEX_Assets (per $1000 Assets)	53,758	59.10	71.84	16.48	36.49	72.00
HI	53,758	0.22	0.17	0.10	0.17	0.28
INST	53,758	0.26	0.31	0.00	0.10	0.49
Z-Score	53,758	1.13	0.96	0.52	1.00	1.54
Tobin's Q	53,758	4.03	9.73	1.12	1.62	2.85
Debt_Assets (per $1000 Assets)	53,758	331.73	604.76	27.93	203.17	391.60
ROA	53,758	-0.19	1.08	-0.08	0.09	0.15
Net worth-to-assets (per $1000 Assets)	53,758	208.10	1,548.08	287.28	488.44	697.85
Current ratio	53,758	2.90	3.63	1.10	1.84	3.16
Interest-to-assets (per $1000 Assets)	53,758	42.37	118.54	3.50	15.04	32.54

Note. Summary statistics for the OLS sample of U.S. non-financial firms from 1996 to 2008 available at http://faculty.chicagobooth.edu/amir.sufi/.

Panel B: RDD sample

Variable	N	Mean	S.D.	P25	Median	P75
Pat	6,280	2.08	9.93	0.00	0.00	0.00
Cite	6,280	2.35	7.63	0.00	0.00	0.00
Violation dummy	6,280	0.26	0.44	0.00	0.00	1.00
R&D/Assets (per $1000 Assets)	6,280	24.63	72.82	0.00	0.00	17.81
Ln(AT)	6,280	5.50	1.64	4.34	5.46	6.61
PPE_Assets (per $1000 Assets)	6,280	321.99	244.09	129.31	252.06	462.90
CAPEX_Assets (per $1000 Assets)	6,280	71.17	81.68	22.56	43.09	82.34
HI	6,280	0.23	0.18	0.11	0.18	0.29
INST	6,280	0.33	0.31	0.00	0.27	0.59
Z-Score	6,280	1.36	0.87	0.77	1.19	1.71
Tobin's Q	6,280	1.68	1.33	1.04	1.33	1.89
Debt_Assets (per $1000 Assets)	6,280	273.40	241.23	103.36	248.17	386.06
ROA	6,280	0.10	0.21	0.07	0.12	0.17
Net worth-to-assets (per $1000 Assets)	6,280	452.91	371.76	327.54	473.78	631.20
Current ratio	6,280	2.27	1.84	1.30	1.85	2.68
Interest-to-assets (per $1000 Assets)	6,280	23.67	30.12	7.62	17.42	30.25

Note. Summary statistics of the RDD sample containing all firm-year observations in which a covenant restricting the current ratio or net worth of the firm is imposed in Dealscan during 1996 to 2008.

Table 2
Comparison of High vs. Low R&D-Intensive Firms

Variable	(1) High R&D	(2) Low R&D	(3) Difference	(4) P-Value
Pat	5.41	0.16	5.25***	0.00
Cite	5.36	0.62	4.74***	0.00
Violation dummy	0.23	0.28	-0.05**	0.01
Ln(AT)	5.32	5.60	-0.28***	0.00
PPE_Assets (per $1000 Assets)	231.73	374.00	-142.27***	0.00
CAPEX_Assets (per $1000 Assets)	49.33	83.76	-34.43***	0.00
HI	0.25	0.22	0.03**	0.02
INST	0.35	0.32	0.03*	0.08
Z-Score	1.22	1.44	-0.22***	0.00
Tobin's Q	1.96	1.53	0.43***	0.00
Debt_Assets (per $1000 Assets)	233.24	296.54	-63.30***	0.00
ROA	0.07	0.12	-0.05***	0.00
Net worth-to-assets (per $1000 Assets)	474.10	440.70	33.39*	0.05
Current ratio	2.62	2.06	0.56***	0.00
Interest-to-assets (per $1000 Assets)	20.99	25.21	-4.22***	0.01

Note. Compare the differences in characteristics between firms with high and low R&D expenditures using the RDD sample. High R&D-intensive firms are those with positive R&D expenditures and low R&D-intensive firms as those with zero R&D expenditures.

* p < .10.
** p < .05.
*** p < .01.

Table 3
Univariate Comparison of Violators vs. Non-Violators

Variable	(1) Violator	(2) Non-violator	(3) Difference	(4) P-Value
Pat	1.75	3.53	-1.78***	0.00
Cite	2.08	3.05	-0.97***	0.00
R&D/Assets	0.05	0.08	-0.03***	0.00

Note. Compare the innovation output variables of violating and non-violating firms using a sample of all U.S. and non-financial Compustat firms between 1996 and 2008.

* $p < .10$.
** $p < .05$.
*** $p < .01$.

Table 4

OLS Regression of Innovation Variables on Covenant Violations

	(1) $LnPat_{i,t+1}$	(2) $LnCite_{i,t+1}$	(3) $LnPat_{i,t+1}$	(4) $LnCite_{i,t+1}$
$Violation_{i,t}$	-0.014*	-0.021	-0.015**	-0.017
	(0.058)	(0.104)	(0.047)	(0.213)
Constant	0.475***	0.744***	0.238***	0.476***
	(0.000)	(0.000)	(0.000)	(0.000)
Controls	No	No	Yes	Yes
Year fixed effect	Yes	Yes	Yes	Yes
Firm fixed effect	Yes	Yes	Yes	Yes
Observations	61,866	61,866	53,758	53,758
Adjusted R^2	0.841	0.557	0.849	0.566

Note. The dependent variables, $LnPat_{t+1}$ and $LnCite_{t+1}$, are the natural logarithm of one plus total number of patents and the natural logarithm of one plus the number of citations received per patent, respectively, in year $t+1$. Independent variable $Violation_{i,t}$ is a dummy variable that equals one if a covenant violation occurs in year t for firm i and not preceded by a violation in the previous four quarters, and zero otherwise.

* p < .10.

** p < .05.

*** p < .01.

Table 5
Innovation Dynamics

Dependent Variable	(1) $LnPat_{t+1}$	(2) $LnCite_{t+1}$
$Violation_{t+2}$	0.009	-0.016
	(0.308)	(0.335)
$Violation_{t+1}$	-0.007	-0.015
	(0.448)	(0.420)
$Violation_{t}$	-0.020*	-0.022
	(0.061)	(0.239)
$Violation_{t-1, t-2}$	-0.024**	-0.002
	(0.018)	(0.893)
Constant	0.434***	0.706***
	(0.000)	(0.000)
Year fixed effect	Yes	Yes
Firm fixed effect	Yes	Yes
Observations	55,078	55,078
Adjusted R^2	0.855	0.586

Note. The dependent variables, $LnPat_{t+1}$ and $LnCite_{t+1}$, are the natural logarithm of one plus total number of patents and the natural logarithm of one plus the number of citations received per patent, respectively, in year $t+1$. Independent variables $Violation_{t+2}$, $Violation_{t+1}$, and $Violation_{t}$ are dummy variables that equal one if a covenant violation occurs in year $t+2$, $t+1$, and t, respectively, and zero otherwise. $Violation_{t-1, t-2}$ is a dummy variable that equals one if a covenant violation occurs in year $t-1$ or year $t-2$ and zero otherwise.

* $p < .10$.

** $p < .05$.

*** $p < .01$.

Table 6
RDD Results

Dependent Variable	(1) *$LnPat_{i,t+1}$*	(2) *$LnCite_{i,t+1}$*	(3) *$LnPat_{i,t+1}$*	(4) *$LnCite_{i,t+1}$*
Panel A: Full sample				
$Violation_{i,t}$ (Current ratio or net	-0.054**	-0.039	-0.057**	-0.04
worth) (β)	(0.022)	(0.321)	(0.030)	(0.348)
Constant	0.377***	0.597***	0.028	0.139
	(0.000)	(0.000)	(0.769)	(0.454)
Controls	No	No	Yes	Yes
Polynomial (2)	Yes	Yes	Yes	Yes
Year fixed effect	Yes	Yes	Yes	Yes
Firm fixed effect	Yes	Yes	Yes	Yes
Observations	7,288	7,288	6,280	6,280
Adjusted R^2	0.85	0.639	0.848	0.625
Panel B: Subsample by excluding firms that never had a patent				
Violation (Current ratio or net	-0.166**	-0.143	-0.159**	-0.112
worth) (β)	(0.019)	(0.224)	(0.033)	(0.361)
Constant	1.079***	1.665***	0.162	1.280*
	(0.000)	(0.000)	(0.672)	(0.067)
Controls	No	No	Yes	Yes
Polynomial (2)	Yes	Yes	Yes	Yes
Year fixed effect	Yes	Yes	Yes	Yes
Firm fixed effect	Yes	Yes	Yes	Yes
Observations	2,496	2,496	2,182	2,182
Adjusted R^2	0.797	0.514	0.794	0.494
Panel B: Subsample by excluding firms that never reported any R&D				
Violation (Current ratio or net	-0.179***	-0.165	-0.164**	-0.111
worth) (β)	(0.004)	(0.109)	(0.013)	(0.299)
Constant	0.940***	1.385***	0.633***	1.599***
	(0.000)	(0.000)	(0.010)	(0.001)
Controls	No	No	Yes	Yes
Polynomial (2)	Yes	Yes	Yes	Yes
Year fixed effect	Yes	Yes	Yes	Yes
Firm fixed effect	Yes	Yes	Yes	Yes
Observations	2,753	2,753	2,428	2,428
Adjusted R^2	0.838	0.617	0.837	0.609

Note. In Panel A, we use the full sample consisting of all firm-year observations in which a covenant restricting the current ratio or net worth of the firm is imposed by a private loan contract included in *Dealscan* during 1996 to 2008. We use subsamples after dropping firms that never report any R&D (in Panel B) or never had a patent (in Panel C) during our sample period. *Violation* is a dummy variable that equal to one if a firm's current ratio or net worth falls below the corresponding covenant threshold in any of the four quarters in a fiscal year.

* p < .10.

** p < .05.

*** p < .01.Table 7

Cross-Sectional Variation

Dependent Variable	(1) $LnPat_{i,t+1}$	(2) $LnPat_{i,t+1}$	(3) $LnPat_{i,t+1}$
Violation (Current ratio or net worth)	-0.040		
**I(Has rating)*	(0.354)		
Violation (Current ratio or net worth)	-0.061**		
**I(Has no rating)*	(0.024)		
Violation (Current ratio or net worth)		-0.048	
**I(Has relation)*		(0.117)	
Violation (Current ratio or net worth)		-0.065**	
**I(Has no relation)*		(0.022)	
Violation (Current ratio or net worth)			-0.025
**I(Large syndicate size)*			(0.478)
Violation (Current ratio or net worth)			-0.073***
**I(Small syndicate size)*			(0.007)
Constant	0.057	0.036	-0.009
	(0.547)	(0.707)	(0.931)
All control variables * I (has rating)	Yes		
All control variables * I (has no rating)	Yes		
All control variables * I (has relation)		Yes	
All control variables * I (has no relation)		Yes	
All control variables * I (Large syndicate size)			Yes
All control variables * I (Small syndicate size)			Yes
Polynomial (2)	Yes	Yes	Yes
Year fixed effect	Yes	Yes	Yes
Firm fixed effect	Yes	Yes	Yes
Observations	6,280	6,280	6,280
Adjusted R^2	0.848	0.848	0.848

Note. *Has (no) rating* is a dummy variable that takes a value of 1 if the firm has either (neither) a bond rating or (nor) a commercial paper rating, and zero otherwise. *Has (no) relationship* is a dummy that is equal to1 if the lead bank of the current loan has (never) acted as a lead bank for any loan from the borrowing firm during the previous 5 years, and zero otherwise. *Large syndicate size* is a dummy that equals 1 if the lead bank syndicate consists of 5 or more lenders, and zero otherwise, and a *Small syndicate size* is a dummy that equals 1 if the lead bank syndicate consists of less than 5 lenders.

* $p < .10$.
** $p < .05$.
*** $p < .01$.

Table 8
Covenant Violations and Patent Value

	(1)
Dependent Variable	*Average Patent Value*$_{i,t+1\text{->}t+3}$
Violation (Current ratio or net worth)	0.462**
	(0.045)
Constant	-0.210
	(0.905)
Controls	Yes
Polynomial (2)	Yes
Year fixed effect	Yes
Firm fixed effect	Yes
Observations	5,913
Adjusted R^2	0.526

Note. Patent value is the economic value based on market announcement return at patent grants, which is deflated to 2005 dollars using the Consumer Price Index (CPI) from the Bureau of Labor Statistics. The dependent variable, *Average Patent Value*$_{i,t+1\text{->}t+3}$, which is the average value of all patents applied within a three-years window subsequent to a given sample year.

* $p < .10$.

** $p < .05$.

*** $p < .01$.

Table 9
Unrelated vs. Related patents

Dependent Variable	(1) *No. of Unrelated Patents*$_{i,t+1\text{->}t+3}$	(2) *No. of Related Patents*$_{i,t+1\text{->}t+3}$
Violation (Current ratio or net worth) *(β)*	-0.905**	-0.350
	(0.010)	(0.182)
Wald test β in column (1) = β in column (2):		
F-Statistics	10.57***	
P value	(0.001)	
Controls	Yes	Yes
Polynomial (2)	Yes	Yes
Year fixed effect	Yes	Yes
Firm fixed effect	Yes	Yes
Observations	6,280	6,280
Adjusted R^2	0.921	0.880

Note. The dependent variables in columns (1) and (2) are the total number of unrelated patents and related patents, respectively, within three years after violation.

* p < .10.
** p < .05.
*** p < .01.

Table 10
Innovation Skills in Related Industries and Inventor Turnover

	Violator Mean Difference	Non-violator Mean Difference	DiD estimator (Violator - Non-violator)	P-value
	(1)	(2)	(3)	(4)
Panel A: DiD test on the percentage of related patents by leavers and stayers				
Stayers – Leavers				
% of related patents	0.074***	-0.129***	0.203***	<0.001
(s.e.)	(0.011)	(0.009)	(0.014)	
Panel B: DiD test on the percentage of related patents by new hires and leavers				
New hires – Leavers				
% of related patents	0.227***	-0.042***	0.269***	<0.001
(s.e.)	(0.032)	(0.016)	(0.036)	

Note. We restrict our sample to a window of three years before and after bank intervention for both violating and non-violating firms.

* p < .10.

** p < .05.

*** p < .01.

Table 11

DiD Test on the Percentage of Related Patents by Stayer Inventors

	Violator Mean Change (after-before) (1)	Non-violator Mean Change (after-before) (2)	DiD estimator (Violator - Non-violator) (3)	P-value (4)
Stayers				
% of related patents	0.016**	-0.003	0.019**	0.011
(s.e.)	(0.007)	(0.003)	(0.008)	

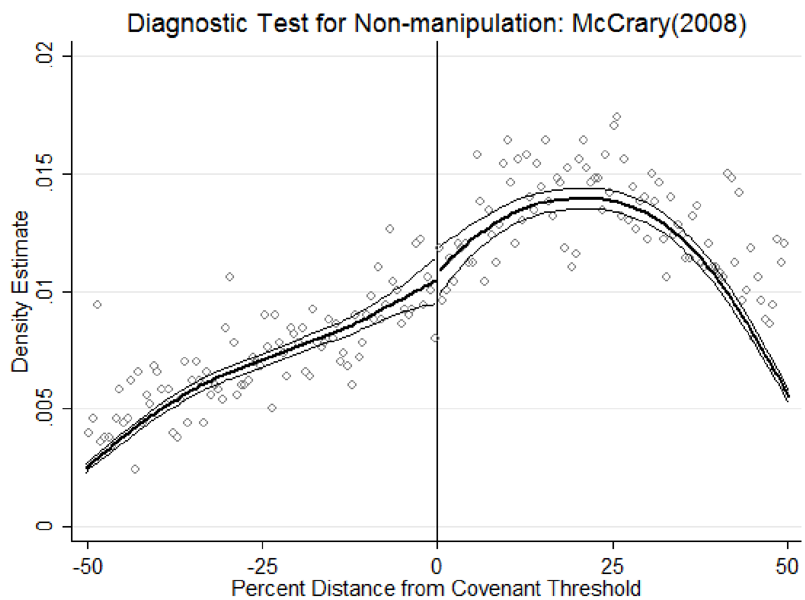
Note. We restrict our sample to a window of three years before and after bank intervention for both violating and non-violating firms.

* p < .10.

** p < .05.

*** p < .01.

Panel A: Current ratio sample



Panel B: Net worth sample

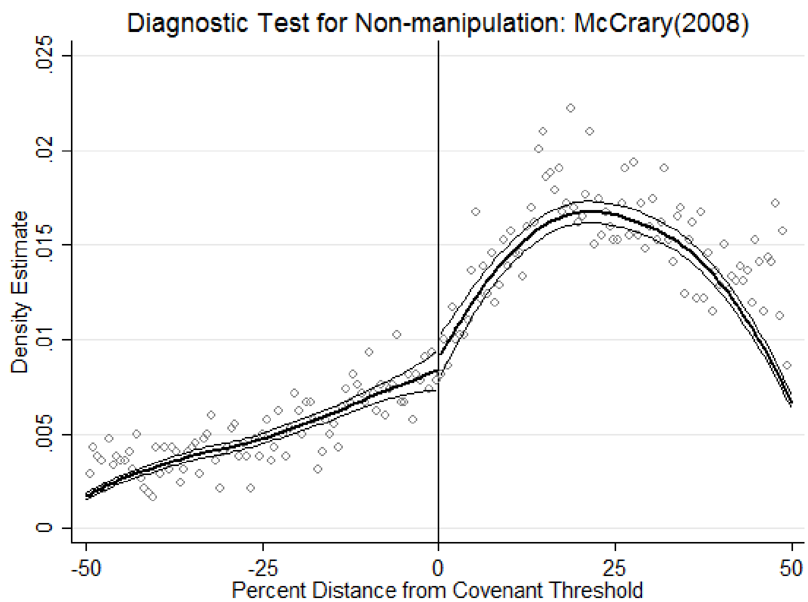


Figure 1. McCrary (2008) Tests around the Two Types of Covenant Threshold

Appendix A: Variable definitions

Variables	Definition
Innovation Measures	
R&D/Assets	R&D expenditures divided by total assets
Pat	Total number of patents filed (and eventually granted) in a given year after adjustment for truncation
Cite	Number of citations received per patent in a given year after adjustment for truncation
No. of unrelated patents	Number of patents that are unrelated to a firm's core business, namely, the number of patents that are not mapped to a firm's main 2-digit SIC industry (or industries).
No. of related patents	Number of patents that are related to a firm's core business, namely, the number of patents that are mapped to a firm's main 2-digit SIC industry (or industries).
% of related patents	Number of patents that are related to a firm's core business divided by total number of patents.
Average Patent Value	Average value of patents as defined in Kogan et al. (2017), deflated to 2005 dollars using the CPI.
Firm Characteristics	
Ln(AT)	Natural logarithm of total assets
PPE_Assets	Net property, plant and equipment divided by total assets
CAPEX_Assets	Capital expenditures scaled by book value of total assets
HI	Herfindahl index based on annual sales in each 4-digit SIC industry
Tobin's Q	Ratio of market value of assets (book value of assets minus book value of equity plus market value of equity) to book value of total assets
INST	Institutional holdings (%), calculated as the arithmetic mean of the four quarterly institutional holdings reported through form 13F
Z-Score	The measure of firms' financial distress risk created by Altman (1968)
Debt_Assets	The ratio of book value of total debt to total assets
ROA	The ratio of operating cash flow to total assets
Net worth-to-assets	Net worth (total assets minus total liabilities) to total assets ratio
Current ratio	The ratio of total current assets to total current liabilities
Interest-to-assets	Interest expenses to total assets ratio
Has rating	A dummy that takes a value of 1 if the firm has either a bond rating (splticrm) or a commercial paper rating (spsticrm)
Has no rating	1-Has rating
Has relation	A dummy that is equal to1 if the lead bank of the current loan has acted as a lead bank for a loan from the same firm during the prior 5 years
Has no relation	1- Has relation
Large syndicate size	A dummy that is equal to 1 if lending syndicate consists of more than 4 lenders.
Small syndicate size	1-Large syndicate size

Idiosyncratic Volatility	The standard deviation of the difference between monthly return of stock and market return over the given fiscal year
Abnormal Current Accruals-DD	Annual abnormal current accruals computed based Dechow and Dichev (2002) and whose derivation closely follows that found in Bharath, Sunder, and Sunder (2008). Total current accruals in year *t* (sum of minus the change in accounts receivables, the change in inventory, the change in accounts payables, the change in taxes payable, and the change in other current assets) scaled by total assets in year t, are regressed on cash flows from operations in year *t*, cash flows from operations in year *t-1*, and cash flows from operations in year *t+1*, which are all scaled by total assets in year *t*. We run the regression using all firms in each Fama-French 48 industry in a given year. Annual abnormal current accruals are the residuals from the regression model.
Abnormal Current Accruals-TWW	Annual abnormal current accruals computed based Teoh, Welch, and Wong (1998) and whose derivation closely follows that found in Bharath, Sunder, and Sunder (2008). Total current accruals in year *t* (sum of minus the change in accounts receivables, the change in inventory, the change in accounts payables, the change in taxes payable, and the change in other current assets) scaled by total assets in year t, are regressed on the inverse of total assets in year *t-1*, and the change in sales normalized by total assets in year *t-1*. We run the regression using all firms in each Fama-French 48 industry in a given year. The parameter estimates from these regressions are then used to compute predicted current accruals for each firm. One modification is that the second regressor from the regression is replaced by the difference between the change in sales and the change in accounts receivables scaled by total assets in *t-1* for the computation of predicted current accruals. Abnormal current accruals are calculated as the difference between the actual current accruals and the predicted current accruals.